



*Bowditch* (with the verbs)  
REMARKS *of H. I. Bowditch*

RELATIVE TO

DR. PAINE'S COMMENTARIES

UPON

THE WRITINGS OF M. LOUIS.

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BOSTON:

D. CLAPP, JR.—184 WASHINGTON STREET.

Office of the Medical and Surgical Journal.

1840.

## P R E F A C E .

THE following remarks were originally published in the BOSTON MEDICAL AND SURGICAL JOURNAL. I wrote them under the influence of the keen feelings produced by the unfairness, as I thought, of Dr. Paine, in his criticisms upon Louis's writings. A German monarch, it is said, though he has established a strict censorship of the press, allows all huge volumes to circulate with perfect freedom—knowing, as he does, that such will be read by few. Perhaps it would have been as well to have allowed Dr. Paine's works to remain unnoticed, except by the few, who would, I doubt not, soon have discovered their value ; but I felt that an absent friend had been attacked unjustly, and I could not forbear rising and doing something towards his defence. At the same time I do not wish any one to suppose that I offer these few pages as a complete review of Dr. Paine's work, even so far as M. Louis is concerned. The remainder of his Commentaries I leave to others.

H. I. B.

*Boston, Sept. 16, 1840.*



## REMARKS.

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DR. PAINE's volumes\* are heavy tomes indeed, so far as size is concerned—but in examining them with reference to the special object we have in view, we have been forcibly reminded of the mushroom. We have seen other volumes equally large and containing as much dogmatism, which were intended for the destruction of heresies—but notwithstanding the zeal and learning of the opponent, the damnable heresy has risen triumphant. We have no fears that Dr. Paine's volumes will have much effect; but though we have no fears for the foundation of Louis's main ideas, viz., 1st, the accurate observation; 2d, recording of cases; 3d, the analysis of them by tables—still we feel unwilling to allow any writer, under the pretence of examining the philosophical views of M. Louis, to traduce his character. If all that is said by Dr. Paine of the former physician of La Pitié be true, M. Louis deserves to be treated not merely as one wholly unworthy of confidence in medicine, but as an individual of a base private character. We use harsh terms, we allow—but what worse accusation can be brought against any person than that he is untrue, that he is willing, for merely selfish motives, to mislead the medical world? The whole tenor of Dr. Paine's criticisms upon Louis's writings leads to such an accusation.

Dr. Paine never suffers his reader to lose sight of the main object of his two volumes, viz., a violent attack upon the numerical, or, as he chooses to call it, the anatomical school. Consequently, there is scarcely a hundred successive pages in either volume, in which this opposition does not manifest itself—and in the second volume, a whole chapter of 134 pages is dedicated to "The writings of Louis;" and after quoting from books of every nature—some of which have as much connection with the subject as Mother Goose's Melodies would have with an introduction to a dissertation on the writings of Franklin—he concludes, with a self-complacent stroking of his beard, thus—"If what we have now said

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\* Medical and Physiological Commentaries. By Martyn Paine, M.D. A.M. 2 vols., 8vo. pp. 716—814=1530. New York: 1840.

of the estimation in which the fathers of medicine have been held by all learned successors should, in some measure, counteract the growing prejudice against this source of much of our best experience and many of the best principles in science, we shall consider ourselves justified in having made this defence." Oh that the venerable forms of Hippocrates, Galen and Celsus could appear and duly thank their "learned successor," Dr. Paine, who in this 19th century thinks that his mission is to defend their memories from the attacks of the "bigoted numeralists"! How much ought medicine to be grateful that its fathers have been preserved from oblivion by such a cogent writer!

But let us commence with the special object of our labors—a review of Dr. Paine's ideas upon the effect of Louis's writings, and of the numerical (or anatomical, according to Dr. P.) school.

The first sign we have of the terror of our commentator in consequence of the prevalence of the writings of the "anatomical school," appears in Vol. 1, when criticizing Marshall Hall's views of venesection, for the London and French pathologists are both classed under one head, although in our opinion entirely distinct characters. However, we will not quarrel with his classification of authors. Dr. Paine quotes the following from Armstrong's Lectures on Fever. The quotation will serve to show the "generalizing" powers of the Dr., as well as his dislike for Louis. "A patient, at the point of death from acute inflammation of the pleura and lungs, was bled to the extent of 50 ounces, when he obtained no relief. If we had stopped here, in two hours the patient would have died. After abstracting six ounces more blood, syncope came on, from which he recovered convalescent." We might complain of some dogmatism here, but listen to Dr. Paine: "If this patient had been bled in an erect posture, and from both arms, and had syncope followed the loss of 15 or 20 ounces of blood, it is scarcely probable that he would have been saved. Here the importance is *fully shown* [no generalizing here, we presume], not only of abstracting a certain quantity of blood, but of obtaining a full impression from the cerebral influence, in many cases of inflammatory affections," &c. Dr. P. continues, and speaks of Marshall Hall's recommendation not to bleed to perfect syncope, as being erroneous. In a note to all this, he says that he (Dr. Paine) has known many to die "from neglect or the inefficient use of" the lancet, since our author's [Dr. Hall's] and M. Louis's works have been extensively circulated amongst us. (p. 230.)

We have a few remarks to make. Considering our commentator is so very wroth, as we shall see hereafter, at Louis's love of generalizing from



one or two cases, we think it very strange that Dr. P. should not merely draw an inference from one case in regard to the effect of bleeding upon pneumonia, but likewise that the same influence is exerted upon "many cases of inflammatory affections." We really cannot see any *proof* of the truth of this proposition, though Dr. P. of course expects us to take his assertions, as he kindly consents to take Dr. Hale's assertion for the truth of the results of 197 cases. (Vol. 2, page 690.) Again, how does he know that Dr. Hall's views are wrong? Has he tried Dr. H.'s plan and found it wanting? If so, let us know the *number* of his facts *pro* and *con*—for we are unwilling to take the assertion of any man.

We may make the same remark in reference to his cases in which death occurred from want of care in bleeding. We want to see some of his cases, in order that we may compare them with some in which bleeding was omitted, or "perhaps used inefficiently," and yet the patients recovered. We would not, however, have any one suppose that we disapprove of venesection in inflammatory diseases. Far from it. Our own experience, even if Louis's cases did not prove it, would convince us that this remedy has very great influence in alleviating the general symptoms of inflammatory diseases of the chest. But we have never seen an acute inflammation "strangled" by it.

Again. The *tongue*, that favorite organ for doctors to look wise over, and therefrom divine the state of the system, becomes next the source of trouble, because, forsooth, *according to Dr. Paine*, Louis says that the indications to be drawn from the state of the tongue are "the least important" of any. (Vol. 1, Fevers, page 238.) But we beg the reader to mark well, though this is but the commencement of misstatements, *Louis never stated this*, that we can find, in his work on typhus, or in fact in any of his works, so far as we can discover. He states, and to our mind, proves, that there is no necessary connection between the condition of the tongue and that of the stomach (Vol. 2, page 55 to 90); but that certain secondary lesions are consequent upon any febrile excitement, and lesions of the tongue are among them. Louis never even said that the indications from the tongue were *unimportant*, but merely that from the tongue alone we cannot infer so much as is usually supposed, in regard to the general system, and that it does not indicate the condition of the stomach.

The next remark we have to notice, is this:—(Vol. 1, p. 282) "We shall see that Mons. Louis and his followers have little or no confidence in the curative effects of bloodletting in pneumonia, and some other equally severe inflammations." Let us see how the facts stand.

Louis says that from his own investigations he is led to believe, "1st, That bloodletting *has a happy effect on the progress of pneumonia*; that it *shortens its duration*; that this effect, however, is much less than has been commonly believed, but that patients bled during the first four days recover, other things being equal, four or five days sooner than those bled at a later period. 2d, That pneumonitis is never arrested *at once* by bloodletting, at least not on the first day of the disease. If an opposite opinion is maintained, it is because this disease has been confounded with another, or because in some rare cases the general symptoms rapidly diminish after the first bloodletting. But then the local symptoms, crepitation, &c., for the most part, continue to be developed not the less for this evacuation."\*

We are fully convinced, from examination of the physical signs, that the local disease goes on when the general symptoms are very much improved by venesection. Now Louis would willingly allow that by subduing the fever we do much towards saving our patient, though the local disease apparently progresses—in the same way that erysipelas progresses after the first febrile action is subdued. But we do not see why Louis or his followers should be considered as "having little or no confidence" in bloodletting, because they have less faith than some others.

Moreover, let us look at the appendix to the American edition of the pamphlet on bloodletting—and which our author quotes with approbation, though it was written exactly according to the numerical method. "But this would not be representing the subject in a light sufficiently favorable for our remedy. \* \* \* Again—so that the advantage derived from *bloodletting* in our practice is greater than that derived from the same treatment in the hands of M. Louis."—(*Bloodletting. Appendix by Dr. Jackson.*)

Our author progresses in his zeal, and devotes three pages (Vol. 1, p. 293) to the improprieties of the numerical school. "M. Louis, in his 'numerical' treatise on bloodletting, endeavors to set aside the practical results of all other eminent men, whose observations probably were not less accurate, though not reduced to a tabular form. The latter observers had found themselves more usefully employed in giving their whole time to the study of nature, and in recording general facts and general results, or in presenting examples in detail, which should most clearly illustrate the ordinary conditions of disease, and thus form the luminous basis of general principles."

Study of nature! forsooth. We presume, then, that it was from

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\* Bloodletting, Bowditch's Translation, p. 48.



pure folly, for a mere pastime, that Mons. Louis, after having been many years in actual practice, gave up his business and entered as clinical aid to his friend Chomel—and at La Charité devoted his days. Here was no studying of nature! It is much easier to write commentaries and talk about the study of nature, than it is to observe accurately. But really, when one denies Louis the credit of having recourse to the strictest study of nature, we must smile at the critic. He knows not the man he is dealing with.

Again—"It is the complaint of the 'numerical school,' that general affirmations cannot be trusted without the tabular view before us." Dr. P. is correct in the first part of this sentence. The adherents to the numerical school say we have had full enough of such words as "very often," "frequently," "almost always;" we want something more definite. Give us your numbers. But numeralists will be, by no means, satisfied with *all* tables or all numbers; they wish to see a book which bears internal evidence of being supported by well-investigated facts, and facts observed, as far as possible, without bias. Many times has it been repeated by the apologists of the numerical method, that tables from ill-recorded facts will lead to error. The numeralists must have an analysis of facts recorded as they actually occurred, and at the time of their occurrence, or they will not be satisfied. Dr. P. complains that thereby the honesty of all previous medical writers is called in question. Does the chemist trust to his memory when making his different analyses? Does not the astronomer record at the moment of observation, and analyze afterwards? Is this discrediting the character of previous observers? This is what Louis wishes the physician to do. No man can remember all his cases. He recollects merely those that are most prominent; hence arise errors in his writings in the closet. But Dr. P. in conclusion, says, "this method, while it offers the general results, supposes that the figures of one man are as full of accuracy as those of another." We differ from this opinion in toto (as may be inferred from our previous remarks); and we wish not to be exclusive, but we really believe that some men are from their constitution incapable of "observing nature" accurately, and the consequence is that all tables made by such persons we should be sceptical about. Already in France such persons have arisen. Our commentator quotes from them.

We might criticize more on these pages, but we will terminate with only one assertion which our author makes—"Others who have carried out M. Louis's 'numerical method,' have come to entirely different results." Every science is progressing; especially is this true in France,



where there is more intellectual activity at the present time than in any other country. One law is good until a higher one is discovered; when that higher one is found out, the lesser of course is put aside. Louis, in his preface to the work on typhus, says, "The best book is good only in relation to the epoch at which it appears, and another must be anticipated that will be more exact and more complete." This we presume every reasonable person would admit. Even now Louis would not be satisfied with the want of minuteness of some of his former observations. Moreover, the attention of the scientific world is always more or less strongly bent to one object. Every tyro now has his microscope, and hopes to learn the arcana of the human frame. In a few years this will be supplanted by another method of investigating. If, in the process of time, facts may be elicited which may clash with some of Louis's results, we ought not to blame him—much less the method by which he arrived at his results.

We now approach the most important part of our author's volumes—for it is evident that Dr. Paine regards M. Louis as the most formidable opponent of medical truth of the present era, and therefore a chapter in the second volume, of more than 100 pages, is devoted to the "Writings of Louis." Four of these pages are occupied with quotations from many authors, and such a medley we have never seen or read of before, save, as Shakspeare has it:—

" Eye of newt and toe of frog,  
Wool of bat and tongue of dog,  
Adder's fork and blind worm's sting,  
Lizard's leg and owlet's wing."

Our readers may think that we give them an incorrect idea of the matter; and the quotations, it is true, are most of them extremely good—but their inapplicability to the main subject is what we object to. Plato and Lord Byron, Hippocrates and Cowper, Baglivi and Pope, are all simmering together with Louis and his contemporaries.

Our author commences thus heroically and self-complacently.—"In approaching the works which we have selected for the subjects of this commentary, we have been actuated by various motives. These will appear, from time to time, as we advance with our undertaking. But we may say now that we have especially in view an exhibition of the ascendancy which false philosophy may obtain, in the intricate science of medicine, at the most intellectual era of man, and to exemplify the inductive and practical results which spring from morbid anatomy when assumed as a paramount guide in pathological inquiries."

Before we pass a step farther, we wish to deny entirely the truth of the assertion, that Louis or the numerical school do make pathological anatomy a *paramount* guide in pathological inquiries ; and Dr. P. must know little of the matter when he accuses Louis of thus using it. We must say that we are devoted lovers of the plan originally proposed by others, but first fully developed by Louis, viz., the Numerical Method. We hereby give in our faith, and believe that as alchemy taught much of chemistry, so medicine gained much from the early fathers of medicine ; but as chemistry has made rapid strides of late years from a more philosophical method of study, so medicine will gain under the numerical method. While we thus declare our entire faith, we have a right to protest against the assertion by Dr. Paine, that we depend upon pathological anatomy for our entire knowledge of diseases. We look upon pathological anatomy as only one means of deciding the question, and not more important than symptomatology. They stand upon a par ; one explains and is connected with the other, and the man who neglects either is a *one-sided* philosopher and will be wholly incapable of any general views. Louis says\*—"I do not fear to say that pathological anatomy has been neither too much boasted of, as some declare, nor too much depreciated, as others say, but its uses have often been little understood. It is a mode of explanation which no other can supply. It is no other thing, but it is certainly of much worth, and because it is one mode of learning about diseases, a mode of verification applicable to all diseases, it seems to me we ought not to make it a science by itself, any more than diagnosis or prognosis." In other words, Louis would use it as *one* of the means of arriving at truth, but not as the sole or fundamental one.

But we go still further, and declare that there is no one from whom we could have learned more real diffidence in anatomical alterations than from M. Louis. In his lectures and at the bed-side of the patient, he is perpetually reminding us that there is something which escapes our senses, even with the most minute investigations—and for the truth of this, we appeal to any one who has followed this author in his visits, or has had personal intercourse with him. But as our readers may wish for some more tangible proof, we quote the following remark of his when speaking of pain, loss of appetite, febrile symptoms, lassitude, &c., as precursors of all *local* diseases. "We should be obliged to refer the commencement of the disease to the period at which these symptoms

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\* Proper method of examining a patient. Bowditch's Translation from Mem. de la Soc. Med. d'Observation, Paris, 1837. Dunglison's Med. Intel., p. 160.

first made their appearance, and to draw the conclusion that an affection wholly local in its appearance may be preceded by general symptoms *which can neither be explained nor referred entirely to the local disease, even when they arise at the same moment.*"—(*Ibid.* p. 154.)

It would seem, then, that our commentator raised up nothing but a spirit; and we find him fighting as a fundamental point of the numerical method, a chimera of his own brain. This two-fold error of supposing the numerical and "anatomical" schools identical, and that the former trusts to pathological anatomy as the ground work of its system, runs through the whole of the hundred pages of criticisms. Alas! that there should be so slight a foundation for the following pathetic exclamation, when speaking of the present reputation of Louis's method, &c. "When after ages shall look back upon this dark spot on the brightest escutcheon of the world, it must be regarded without sympathy and as an act of voluntary humiliation."

But let us come to details. Dr. Paine seems to think himself called upon to defend the reputation of Chomel, and assures the reader that Chomel acted a very subordinate part, and should be in no respect associated with our author's performance in his work on fever, or typhoid affection. We presume that there are scarcely two men in Paris more intimate, and more mutually respectful, than Louis and Chomel. They are very near friends. But it remains for Dr. Paine to find out that Chomel ought to be ashamed of his co-laborer; at any rate he endeavors much to persuade his readers that Louis was the acting man and Chomel had nothing to do with the matter. Now, the facts are these. Louis, disgusted with the uncertainty prevailing in all branches of medicine, returns from the foreign country where he was settled, resigns his professional duties, enters the wards of the hospital which are under the care of his friend Chomel, determined, like a sincere seeker for truth, to record the facts he sees there with perfect indifference as to the results to which a future investigation would lead him. Chomel bids him enter and pursue his studies freely; but Chomel (notwithstanding Dr. Paine takes it upon himself to declare to the contrary) is *always the chief physician*.

In order to prove how much ashamed Chomel is of his friend, and of the work which originated in his own wards, we quote the following from his *Leçons de Clinique Medicale*, Vol. 1, which treats of typhoid fever. "In speaking of the history of this disease, upon which the labors of Messrs. Prost, Petit, Serres, Bretonneau, and *especially the model work* by Mons. Louis, have thrown much light," &c. (p. 2).



In quoting Louis's results, he says, "Those which Mons. Louis has given in his *learned* work upon the subject" (p. 76). Again he says—"Also in the ten cases of this kind which were collected and published by M. Louis, *this able observer*," &c. (p. 128). And finally, as if it were written especially to refute Dr. Paine, we find the following—"If we judge of them from more numerous cases observed by M. Louis, in our service [or wards] at La Charité," &c.

We perhaps have spent more time than was necessary upon this, but as our author thought it necessary to devote two pages to the subject we were unwilling to pass it by unnoticed. Moreover, there is one assertion which is wholly untrue, and as it bears upon the point, we quote it. "It is also of constant recurrence, '*I prescribed*'"—meaning thereby to state definitely that Louis prescribed. Now there is no such expression in the work on typhoid fever—and we challenge Dr. P. to cite it. Dr. Paine thinking (we know not why) this point a very important one to be settled, returns to it at the latter part of his chapter, and gives another specimen of his unfairness. We quote from page 800—"Although it is everywhere apparent that he [Louis] is alone responsible, we will now state his direct affirmation to this effect. '*We abstained* from blood-letting, &c.' " Now we have always thought that common honesty of heart would tell a man that he should look at the original text, and not trust to any translation. Dr. Paine attempts to prove Louis a liar (we are aware of the meaning of the word), by quoting a translation; whereas, if he had taken the trouble to look at the original, he would have found that he by no means could prove what he wished. "*On abstint*" are the two words in the original French. Now we appeal to any one who has the merest smattering of that language, and ask whether Dr. Paine has done rightly. It is, however, quite in accordance with the greater part of the whole chapter.

But enough of this. These are mere trifles in comparison with the false statements that follow.

Having premised thus much about Chomel, our author continues, and finds an inconsistency between the motto from Emile and the advertisement (page 686 Com.). Louis says (quoting from Jean Jacques Rousseau), "I know that truth lies in facts, and not in the mind that judges of them," &c. "The reader" (thus remarks Dr. P.) "will ultimately feel the whole import and intended force of the foregoing paragraph; and whilst our author is everywhere engaged in drawing the most unqualified generalizations from these limited observations, the reader is as constantly drawn into the belief that our author is only con-

cerned about the exhibition of *rigorous facts*. And yet be it said that our author, to carry the only purpose which could render these ‘138 observations’ in the least instrumental to his fame beyond the day of their promulgation, announces in his ‘*advertisement*’ what is everywhere the final object of his analytical investigations, ‘the hope of arriving at *conclusive results*.’”

The above is a fair specimen of Dr. Paine’s method of quoting from our author. The following is the passage to which Dr. P. refers. “*Bien que mon ouvrage ne soit pas un traité de l’affection, qu’il ne doive être considéré que comme l’exposé des faits qu’j’ai recueillis sur cette maladie,\* l’espoir d’arriver à des résultats concluants m’a conduit comme on vient de voir à l’analyse d’un grand nombre de faits relatifs à des affections d’une autre espèce.*” (Although my work is not a treatise on the typhoid affection, as it ought to be considered merely an account of the facts which I have observed relative to this disease, the hope of arriving at conclusive results has induced me to analyze a great number of facts relative to other diseases.)—*Pref. to Typhoid Fever*.

Now we ask, has Dr. Paine acted fairly in quoting, as he has quoted, a few words from the middle of a sentence? One would suppose, from the quotation, as given by this commentator, that Louis meant to say that he considered he had settled the whole affair so far as typhous fever is concerned—that he had arrived at conclusive results, and therefore there would be no need of further investigations. Whereas he expressly states that he does not consider his work “a treatise upon the typhoid disease,” but merely an account or summary (*exposé*) of the facts which he had observed. But let us search for Louis’s opinions upon this point elsewhere. In his preface to his volume of *Memoirs*, he says, “Although the number of facts we have collected is far from being sufficient to definitely fix this proportion, still they may aid in the attainment of this object—and if every one followed the same plan, we could discover the truth after a few years—and the same method, continued for a still greater lapse of time, would enable us to decide,” &c.† In these two quotations there is certainly sufficient deference paid to others, and not any extraordinary degree of arrogance on his part.

It must be always kept in mind that Louis collected his facts in Paris,

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\* We must add Louis’s note to this passage, and the following is a literal translation.

“This is likewise the reason why I did not think it necessary to examine the opinions of those authors who have devoted themselves most successfully of late to the study of fevers. I would add that I could not have done so without increasing the bulk of my volume; and moreover the time for this examination is perhaps not yet come, and it would be done perhaps better by another than by myself. Nothing less than all these considerations could have prevented me from using this opportunity of rendering a just tribute of respect to my brethren, and of repaying with my thanks those who have shown so much kindness to me in their publications.”

† *Memoires, ou Recherches Anatomico-Pathologiques, &c. par P. Ch. A. Louis. Paris: 1826.*

and from them deduced his results. He did not, because he could not, observe in England or America. Hence all that Louis or any of his friends would contend for, is, that an analysis of his facts gives the results for the disease known under the name of typhus or typhoid fever in Paris.

Our commentator proceeds, and states his astonishment at finding Louis generalizing too quickly. "The first thing," he says, "that excites our surprise, is the broad affirmation that a lesion of the glands of Peyer may be taken as the anatomical characteristic of typhoid fever, because, &c. \* \* \* \* \* Here, in this second generalization, is one important foundation of our author's renown. He had thus identified himself with an unexplored disease, and presented it as an isolated affection which may always be distinguished from the group with which it is allied by a comparatively unimportant lesion of structure. \* \* \* \* \*

But let us inquire how far our author has been sustained in the foregoing generalization by the observations which he has elicited from others. We allow that this may seem a work of supererogation to the most enlightened of the profession in Europe, but it is necessary to the purposes of this essay that the subject should be considered." How truly condescending on the part of our learned commentator to be thus willing to instruct us simple Americans in what, were we all as learned as Dr. Paine, or as "the most enlightened of the profession in Europe," we should doubtless be very well versed! But we will not quarrel with the self-complacency of the doctor, but proceed to criticize some of the quotations from the authors whom he cites to prove his position.

First, we shall speak of "this unimportant lesion of structure." Among the ablest and worthiest, and the one upon whom Dr. P. rests his greatest hopes, is Chomel. In the first place Chomel everywhere, in the volume before us,\* speaks of Louis as the "savant," and "able observer," and of his work on typhous fever as "a model." And what are his results? Let us listen to Dr. Paine's *description* of them, and afterwards learn the TRUTH. He says (p. 688) that Chomel "has seen the same alteration of the glands of Peyer as attend typhus, in scarlatina and other affections (*sujets morts d'affections differentes.*)"

We are sorry to see such a lamentable deficiency in the fairness which we expect in one who quotes. The reader doubtless will suppose, from what we have extracted from Dr. P.'s remarks, that Chomel believes that the peculiar lesion of the intestinal follicles ascribed by Louis to the typhoid affection, can be found in many diseases. Now we deny



that Chomel ever said so, or meant to be understood so to say; and we assert that he declares exactly the contrary, and that it is Dr. Paine's garbled quotation that has led the reader into error. That he has made exactly the same inferences that Louis has, we do not wish to state. What two men are there that will agree wholly, when there is any room for difference of opinion? But the differences in the present case do not affect the point under consideration. Chomel divides the anatomical lesions into those that are *constant* and those that are *accidental*. In the first class are reckoned, as is done by Mons. Louis, the lesion of the follicles of the small intestine (with this difference, that under this expression Chomel includes both Peyer's and Brunner's glands), and he continues thus: "We conclude from these researches, depending upon numerous observations, agreeing in the most important particulars with those made by Mons. Louis in Paris, and Dr. Bright in England, that the alteration of the intestinal follicles is a condition *wholly peculiar* (tout à fait particulier) to the typhoid affection, the different periods of which we can follow as we can *those of an abscess or a cutaneous exanthem*."—(*Leçons*, page 222). Truly this looks very much as if Chomel thought the follicular affection, in the typhoid disease, was "a comparatively unimportant lesion of structure."

But our author says, Chomel has seen the same alterations in other diseases. We deny even this. On the contrary, in the three diseases mentioned by him as having something similar, viz., cholera, phthisis and scarlatina, he makes two very evident distinctions—for instance in cholera: 1st, "There is less prominence of the follicles than in typhoid. 2d, The lesion is the same at every epoch of the disease—whereas there is a regular change in them in fever." In reference to scarlatina, Chomel says that the disease of the follicles resembles that in cholera, and therefore is not like that of the typhoid disease. Moreover, let any one peruse the cases collected by Jackson\* while the cholera was raging in Paris, and he will find nothing to confirm him in the idea that in the two diseases is the same anatomical change. In cholera the congregated glands, or Peyer's patches, were not diseased in comparison with Brunner's or the isolated follicles. With regard to phthisis, Chomel makes similar statements. We have ourselves had some opportunities of comparing the intestinal lesions of phthisis and typhus—and we must say that the idea of confounding the two when we examine the whole track of the canal, never could have been entertained by us. The anatomical differences are much more

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\* Cases of Cholera collected at Paris, &c., by James Jackson, Jr., 1832.

distinct than many cutaneous affections—for instance, measles and scarlatina.

But in order to lay the whole truth before the reader, we must inform him that Chomel announces that he does not agree with Louis in regarding this affection of the patches as absolutely necessary to the typhoid affection, because he thinks that, 1st, sometimes it is absent in cases where the symptoms are those of the typhoid disease; and 2d, because sometimes the severity of the symptoms does not accord with the slight lesion of the follicles. This opinion is drawn not from his own facts, but from those of others. But he seems in doubt about any previous step in the disease, and as he is ignorant he is willing to confess it, and waits until further facts are collected. Dr. Paine may think he has gained his end, and that Chomel and we both allow that Louis generalized too quickly—and that in stating the anatomical characteristic of the typhoid fever to be a lesion of Peyer's patches, we declare that the symptoms are dependent upon this change of these patches. Now let us examine Louis's works and see what he says upon the subject; and first, we must say that in stating the foundation of Louis's assertions, Dr. Paine, as usual, gives an unfair impression of his labors, and leads the reader to believe that all the cases of any disease that Louis examined in order to arrive at definite conclusions in reference to the characteristic lesion of the typhoid disease, were "50 cases of acute disease having certain other analogies, and 83 other cases where these analogies are said to have been more or less wanting." After speaking of the state of doubt in which physicians were in reference to fever—some calling it a gastro-enteritis, others a putrid adynamic, ataxic and typhoid fever, Louis continues thus: "In order to make up my mind upon a question which simple discussion would not tend to elucidate, I examined and recorded, between the years of 1822 and 1827, the histories of *all the patients* affected with acute disease, that were admitted to the hospital of La Charité in the apartments under the supervision of Mons. Chomel. During this period I obtained, with the exception of some imperfectly recorded facts, 138 observations of the typhoid fever, 50 of which related to individuals that died. I analyzed both, and in order to know, among the numerous lesions found in those who died, those that were peculiar to the typhoid affection, I compared them with the alterations observed in consequence of other acute diseases, in 83 subjects, whose cases I carefully recorded. I did the same when examining the symptoms in patients affected with the typhoid disease or any other acute affection terminating fatally, or by return of health. So

that in fact I have analyzed the alteration in the viscera of 133 subjects who died, and the symptoms of nearly 900."

One would think that these facts were sufficient to enable one to come to some definite (we will not use "conclusive," as it offends our commentator so much) results. "In my analysis," continues Louis, "I have wholly left out any facts which were not sufficiently exact—and when I have deduced any consequences, I have always kept before me this idea by the author of *Emile*, 'I know that truth resides in things, &c.'"<sup>a</sup> In a note to this paragraph Louis informs us that he threw aside as incomplete all the "observations" made during his first eight months of devotion to these studies. One would think that the accurate examination of about 1000 cases, and the autopsies of 1-10 of them, would have enabled any accurate observer to decide whether a lesion was *unimportant* or not.

So much for Louis's data and accuracy of observation of nature.

We now return to the subject of the anatomical characteristics of typhus, typhoid affection, or continued fever. The reader will bear in mind that anatomical characteristics are very different from the causes of disease. The anatomical characteristics are, comparatively speaking, easily discovered by careful examination—and in common, but somewhat inaccurate language, we allow they are called the causes. For instance, Louis says inflammation of the lung is the cause of the various groups of symptoms usually accompanying and giving us information of the existence of that disease. But there is a step still farther back, and which is hidden, for the most part, and to this step must we go for the real causes. These are the most hidden subjects we have to deal with. In the present age, and with our actual knowledge of the phenomena of life, it will be long ere we shall be able to *know* much upon etiology—yet upon nothing do medical authors "tax their imagination for their facts" more than upon this same topic.

The anatomical characteristics of typhus, according to Louis, are of two kinds, *primary* and *secondary* (a similar opinion to Chomel's). In the first rank, Louis places the affection of the congregated follicles (Peyer's patches), because "they were more or less seriously changed in structure in all the patients."<sup>b</sup> But there are other *secondary* characteristics—"Ulcerations of the pharynx and œsophagus having occurred only in a small number of typhoid patients, and in no other disease, may be considered as among the anatomical characteristics of the former, though they are secondary."<sup>c</sup> Now we cannot see how

<sup>a</sup> Louis on Typhoid Fever, Vol. 1, p. 172.

<sup>b</sup> *Ibid.*, p. 383.



Louis could have drawn any other inference. We beg the reader to remember that Louis's work was written from cases collected some time ago, between 1822 and '27, and that he pretends "not to give a perfect treatise on the disease."\* He examines his own cases collected in Paris, and in wards devoted to adults. Doubtless, he never intended to say that there might be no difference observed between the typhus of Paris and that of England or America. He left that for others and future observers to decide. Doubtless, moreover, he would have made a more perfect work had he studied the disease in children and old age, and in other countries. He would have had a larger number of facts. But supposing he had devoted 6 years to children, 6 to the aged, 6 to England, &c. ; his life would have terminated, and we should not have had the "model" work by M. Louis. But ought we to complain of a man who makes inferences from the facts he has in his possession?

Dr. Paine states (p. 169) that the follicular affection is denied to exist by Louis and his followers in genuine typhus. We know not to whom Dr. P. refers under the title of followers ; but as it regards Louis, we deny that he ever made such an assertion, and challenge Dr. P. to produce any proof to that effect.

Dr. P. quotes from Lombard and several English and American writers in regard to the characteristics of fever. Even if it be certain that in the course of fever in England and America there are other lesions than those described by Louis, we can derive no argument from this fact against Louis's results—inasmuch as he limits these results to the disease as actually existing in Paris. The subject, in our opinion, is still sub judice as it regards these characteristics, when fever is viewed as an affection liable to occur in every part of the world ; but this does not lessen our confidence in M. Louis ; and though we may differ from him, we may respect his method of investigating, believing, as we do, that some higher law will eventually reconcile all differences. But at present, we must conclude, both from Chomel's and Louis's researches (82 fatal cases collected during a space of 12 years by two eminent men), that the anatomical characteristics of typhus in Paris are as have been described. But Dr. Paine has no right to use such an expression as this, when criticizing Louis's results. "They are," says Dr. P., "designed for every climate, constitution, habits, and other predisposing and exciting causes." As much right have we to complain of Mons. Louis for having said that ulceration of the intestines in chronic diseases is never found except in combination with tubercles in the lungs. Now

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\* Preface to Typhoid Fever.

we have seen two cases, at least, and if we had resided farther south should probably have seen many more, of chronic diarrhœa from long residence in the West Indies; and in these cases, in which we examined the lungs with the utmost caution, making the minute subdivisions of them with especial reference to Louis's remarks, we found no trace of tubercle, but the intestines were studded with ulcers. We explain the fact of the error of Louis from this circumstance, that the diseases of warm climates rarely, if ever, appear in the Paris hospitals. We never saw one during our residence in that metropolis. We do not, however, consider Louis as being unworthy of confidence because he has chosen to say—"It is now more than 8 years since my Researches [on phthisis] were published, and I have not met, during that period, with a single subject who has died of a chronic disease, and with ulcers in the small intestines, in whom there was not at the same time tubercles in the lungs."\*

But the climax is coming. We cannot forbear smiling to view the overweening self-complacency of our commentator when commencing his plan of "rapidly glancing at the prolific results of those 50 cases of typhus." Observe the bathos! "But it is indispensable to the success of our enterprise, and when we shall have brought them to the solemn consideration of the reader, yet leaving them mainly to his intelligence, we cannot but think that they will be regarded as a fearful beacon to the present and coming generations"! (Page 694.) Let us see how Dr. Paine has succeeded in proving that he deserves immortality for having proved that Louis is an arch traitor to truth, and that his works ought to be regarded as a warning to all future generations!

In the first paragraph after this flourish of "solemn" trumpets, we find a radical error in the statement of Louis's opinions—as follows. "Our author, for instance, has no conception of disease which he cannot trace out through some lesion of structure; and when he endeavors to insinuate the belief that diarrhœa cannot exist 'without appreciable lesion of the intestinal mucous membrane,' he fears that his hypothesis may find some opposition from analogies supplied by the natural conditions of the body." (P. 695.)

Our readers would scarcely believe us if we were merely to state that all this assertion by Dr. Paine is radically *false*, and that in the above paragraph in which Dr. P. *pretends* to quote from Louis's work on phthisis, Louis really *says exactly the reverse of what Dr. P. states that he does*, and that instead "of having no conception of disease which

he cannot trace out through some lesion of structure," he declares, in *this identical paragraph*, that he *does believe in disease of function without appreciable alteration of structure*. Dr. Paine may hope to escape the imputation of falsehood, by using the word "*insinuate*." But let any candid reader read the sentence which Dr. Paine has written, and he would say that Louis really *believed* that diarrhœa could not occur without lesion of structure, but that he was afraid to say it openly, and therefore merely "*insinuated*" it. We must say that we feel indignant when such accusations are made against this author, for they show either wilful blindness or total ignorance on the part of the accuser. But let us see the original and translation.

"Observons que ces sueurs si copieuses indiquent un derangement des fonctions de la peau, aussi remarquable par son degré que par sa durée ; que ce derangement, qu'il soit sympathique ou du à une autre cause, n'en est pas moins réel, et a lieu *sans* alteration sensible de la structure de l'organe qui en est le siege ; qu'ainsi qu'une fonction peut être plus ou moins altérée pendant long temps, sans que l'organe qui en est chargé offre de changement appreciable dans sa texture. Remarquons, encore, qu'à défaut de faits qui prouvassent d'une maniere directe que le devoiement peut avoir lieu sans lesion appreciable de la membrane muqueuse de l'intestine, cela serait à présumer à raison de l'analogie qui existe entre des sueurs copieuses et une diarrhœa plus ou moins forte. Nous ne disons pas evident, parceque, dans notre manière de voir l'analogie ne peut servir qu'à indiquer de nouvelles recherches, à aller à la rencontre des faits, et jamais à les suplier—autrement, ce seroit conclure de la possibilité d'une chose à son existence, ce qui est absurde."—(*Researches on Phthisis*, s. 259.) "We remark that these copious perspirations indicate a derangement in the functions of the skin, as remarkable in degree as in duration ; that this derangement, whether it be the result of sympathy or of any other cause, is not the less real on that account, *and occurs without any* appreciable alteration in the structure of the organ in which this derangement occurs. And thus we find that a function may be more or less seriously altered for a long while, *and at the same time the organ, which is the origin of the function, may present no appreciable change of structure*. We would likewise observe, that in the absence of facts which directly prove that diarrhœa may occur without any appreciable lesion in the mucous membrane of the intestine, we might presume that to be true in consequence of the analogy which exists between copious perspiration and severe diarrhœa. We do not say that this is proved (evident, Fr.), because we think that



analogy serves only to point the way to new researches; it teaches to seek, in a certain direction, for new facts, but it never supplies the want of them; for if it were otherwise, we should deduce the absolute existence of a thing from the simple possibility of such existence, which is an absurdity."

The inference we draw from the above perversion of Louis's words, is one of these: 1st, Dr. P. has wilfully falsified a remark of an author whom he wishes to hold up to scorn; or, 2d, Dr. P. reads so carelessly that he did not observe his mistake. Upon whichever horn of the dilemma the Dr. may place himself, the inferences are not very pleasant for a man who writes such full commentaries upon the medical theories of the day. But our commentator may not choose to think himself placed as we think he is. Be it so, and let us hear him in his future remarks. After a long defence of analogy as a source of evidence, he concludes thus triumphantly! "Thus in the example which our author fears may encroach upon the dominion of morbid anatomy, who is there that will not concede that 'profuse perspiration' arising *from disease* without 'any appreciable lesion' of the skin, is not a substantial ground for induction that 'diarrhœa'—aye, and many other morbid results—may take place independently of any 'appreciable lesion' of structure? And to show you [mark well the Dr.'s earnestness] how analogy may grow into a matter of fact, and in this very instance, we will point you to serous effusions in the brain, thorax, abdomen, where the secreting membranes often exhibit their perfectly normal state."—(P. 695.) Heaven defend medical art from the "*facts*" which grow up in this way. A man has sweated, and no change of structure of skin is observed—therefore, says Dr. P., we are *certain* that a man may have hydrothorax, dropsy of the brain, hydrocele, &c. &c., without evident change of structure in the organs implicated. We must say that we should prefer to examine the chest and head and *see* whether these things are so, rather than to infer that these diseases exist merely from what passes upon the skin. It is a long while since we studied logic under the venerable Dr. H. Would that we could appeal to that learned man. Even he, with all his logical acumen, would be shocked at such unwonted use of analogical reasoning.

We hasten to another instance of our commentator's unfairness. On page 696, he says—"Our author has no difficulty with analogy where a lesion of structure may embellish the philosophy of disease. Thus: 'Analogy,' he says, 'is in favor of what we advance. For, when hæmorrhage occurs in any internal organ, it is almost constantly a symp-

tom of more or less considerable alteration of structure.' From this assumption, he reasons analogically that 'hæmoptysis (with certain exceptions), whenever it occurs, renders tubercles in the lungs infinitely probable.'"

So much for Dr. Paine's assertions. Let us see how the matter *really* stands. Louis commences the paragraph upon hæmoptysis (Phthisis, s. 231) by stating that 57 out of 87 patients had it. He then states, 25 had it copiously. Again he asks (s. 233), "Are we, however, to consider the hæmoptysis, especially when copious, which precedes cough and expectoration, as a precursor of tubercles, or simply as a symptom which reveals their presence?" He then states, that for nearly three years he had constantly asked all his patients in reference to this symptom, and he found, that except in the phthisical patients, and those who had received injuries of the chest, or in whom the catamenia were disordered, none had hæmoptysis. And he continues thus: "We think, therefore, that hæmoptysis, except in the cases above mentioned, at whatever period it may occur, makes it infinitely probable that tubercles exist in the lungs. We do not say that this is certain, because there have been many well-attested facts which are fortunate exceptions." Moreover (he continues, in the next paragraph), "analogy favors this proposition. For when a hæmorrhage," &c. And he terminates the paragraph thus: "But let us cease with these few remarks [that is, reasonings from analogy] which are intended much less to supply facts, than to excite to investigation."

Really, if Dr. P. did not put forth such pretensions to learning and candor, we might apply to him much harsher epithets than those we have already used. Does not that man deserve severe rebuke, when under pretence of stating an argument in reference to a subject, he dares entirely to reverse the order, and uses a remark made by the author for the purpose of exciting others to investigate, as if it were the chief corner stone of the author's argument? We heartily detest such trickery.

Our commentator seems to be unwilling that M. Louis should dare use the word *experience*, unless he has numbers to prove it—so bigoted does he seem to suppose our author is in regard to the numerical method. For instance—on p. 699, we find, "Here is another example of adherence to 'rigorous facts,' and of the uses which our author makes of analogy, where questions of the most vital and general nature are concerned. Thus: 'Experience shows us, that in spite of these striking and indisputable differences between persons most resembling one another, 999 out of

1000 who differ in age, sex, temperament, &c., live on the same food, prepared in the same manner.'—(*Bloodletting*, p. 58.)”

On page 700, the commentator quotes several passages. In reference to one, he says that Louis regards structural disease as the essential pathology of disease. We have already denied this assertion; but we quote the following: “If the senses do not appreciate everything, if there is anything else in the typhoid affection than what the eyesight can discover, such is also the case in almost all internal diseases, which in this respect” (in having something more than *structural lesion*) “are scarcely less mysterious than fevers.”—(*Page 394, Typhoid, Vol. 2.*) Our commentator returns to the charge afterwards (Vol. 2, p. 762), and quotes various passages from Louis’s works, tending to prove (as Dr. P. thinks) that Louis regarded the lesion of Peyer’s patches as the *cause* of the symptoms in typhus. Now the sum and substance of the whole of the quotations may be illustrated thus. A case of very severe pneumonia occurs, accompanied with delirium. Death ensues—chiefly, as Louis thinks, in consequence of the mania. Yet upon examination, we find both lungs extensively hepatized, but no appreciable change in the brain. Louis says, that in the present state of our knowledge we must refer the mania to the disease of the lungs, and not to any material change in the brain. Now it really seems to us that Louis is correct, though we see no sufficient reason for his urging the point so much. We have no doubt that no delirium would have existed had not the man been affected with pneumonia. Just so does Louis regard the delirium in typhoid fever; believing, as he does, that the lesion of the alimentary canal is as characteristic of the typhoid disease, as hepatization is of pneumonitis. In one sense, pneumonia was the *cause* of delirium—yet how different our ideas of causation when it is regarded in this light, from those previous causes which give origin to the whole phenomena of disease; and upon these, hear what Louis says. “The deepest obscurity hangs over the causes of the affection under consideration.”—(Vol. 2, p. 393.)

Below we have another specimen of our commentator’s unfairness, with either a disposition to lead the reader astray, or great carelessness in quotations. After quoting from the Typhoid Fever (Vol. 1, p. 152), “that Louis thinks that the symptoms of disease of the stomach are very obscure”—and in another place “that the mucous membrane of the stomach was more or less seriously altered in the greater proportion of cases,” Dr. Paine triumphantly compares them with another passage (Vol. 2, p. 131–2), and thinks he has discovered an inconsistency



between it and the two previous ones. This sentence is as follows : " It is nearly correct to state that the apparent condition of the brain cannot explain the symptoms of which it is the source, any more than the mucous membrane of the stomach can account for the anorexia and other gastric symptoms in the great majority of cases." Now the reader may think that there is inconsistency. Let him read the following, from Vol. 2, p. 39. " Thus we see that out of 30 subjects from whom I was able to learn anything about the gastric symptoms, twenty had vomiting, nausea, or pains in the epigastrium, and out of these only eleven had any serious alteration of the mucous membrane of the stomach"—in other words, a proportion of one half had symptoms, but no corresponding lesion. Dr. Paine should have quoted this passage, and not have brought together two sentences upon entirely different subjects. But this, as we have already frequently seen, is but too often the course pursued by Dr. Paine

We are almost fatigued with the numerous instances of unfairness—but the following extract affords another specimen. Louis, from examination of the heart, thought it was not inflamed, and he describes it thus. "*At the same time that it was softened, it had less color than usual in many cases. It was of an onion-peel color, which varied in intensity, and* (was generally livid and purplish on its surface as in its substance. The internal face of the ventricles and auricles was, on the contrary, of a deep violet red color) *which color sometimes penetrated beyond the lining membrane, and appeared owing to an imbibition of blood, which it resembled more or less in color.*"\* Afterwards he states that the walls were thinner than usual. Hence he concludes that inflammation did not cause this affection. But Dr. P. quotes the part of the sentence only that is included in the above parenthesis, and when Louis says " that if we know any cause of disease directly the reverse of inflammation, it would be proper to refer this softening to it," the commentator says " our author refers to the absence of pus in the walls of the heart, as a special proof of the foregoing doctrine ;" whereas Louis uses this fact, and likewise the non-existence of pericarditis in any of the cases, as merely considerations to support, in some measure, his previous arguments, and which to any fair mind are sufficient. But, as we have seen in the case of analogy, Dr. P. takes what Louis uses as merely supplementary to the main argument, and puts it forth as the chief groundwork.

Here is another specimen of the misstatements by our commentator.

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\* Italics our own, in order to mark what Dr. Paine suppressed.

We are sorry to be obliged to use such terms towards a medical associate, but nevertheless the truth must be told in this case at least, let it be never so pungent. Dr. Paine (p. 703) attempts to convict our author of something worse than inconsistency in stating that softening, thickening and ulceration may occur without inflammation, when tubercles exist in the alimentary canal; and in order to gain this end, Dr. P. makes these remarks. 1st, His inductions *are founded wholly upon the debris of the body*; 2d, The inductions rest chiefly upon the fact that the foregoing alterations in structure are white in one case and red in the other. And after two or three pages of quotations from Louis's writings, in which additions in the way of comments and subtractions are made to suit the fancy of the commentator, he finishes with a quotation from Cowper's Conversation, and applies it to the "Numerical Method."

"Such continual zigzags in a book,  
Such drunken reelings, have an awkward look,  
And I had rather creep to what is true,  
Than rove and stagger with no mark in view."

"This may be very philosophical;" but let us see *how* our commentator has convicted Louis of inconsistency on this subject. In regard to the first statement above, I would ask how, *à priori*, without "examination of the debris of the body," can we determine upon the existence of inflammation of an organ under the skin? How originally did we arrive at the idea of hepatized lung, except by post-mortem appearances, or, as Dr. P. says, "from the debris of the body?" The symptoms might lead us to *infer* the phenomena of inflammation, but we never could do so unless from previous study of the parts in an inflamed state, and a comparison of this state with the symptoms. So much for this; and with regard to the second, we join issue entirely, and declare that Louis never did propose to decide that a part was *inflamed* from the existence of redness merely. Moreover, as we deny the premises, so we deny all the inferences drawn therefrom in reference to the credibility of Louis; for we beg the reader to observe that Dr. Paine accuses our author "of occasional guarded contradictions," for the purpose of gaining "a reputation for candor," as "that more effectually secures to him a successful propagation of his favorite, though conflicting hypothesis."

This subject will occupy considerable space. Let us first quote from the commentator, and afterwards compare with it Louis's remarks upon the same subject at the latest period of his life, viz., when writing his work on the typhoid disease.

"Here we pause to consider how far our author has supplied any

ground for his principle that softening, thickening and ulceration, of different tissues, sometimes depend upon inflammation, and at other times on an 'exactly opposite condition of disease,' and what, also, is the probable motive for introducing this confusion into the most important branch of pathology.

"In the first place, the inductions are founded *wholly upon the debris of the body*. There is no where, that we have been able to discover, any essential reference to the phenomena of the disease during its actual existence. Even the remarkable similarity of those phenomena appears not to have been held in consideration, in forming the conclusions. Secondly, the inductions rest chiefly upon the fact that the foregoing alterations of structure are *white* in one case, and *red* in the other.—(P. 548, *Andral*.) This may be very philosophical; but let us see what our author thinks of it when he is engaged in reasoning the reader into his problem, and in supplying the appearance of an impartiality which never fails of a prepossessing influence, and carries us along with greater confidence to the never-failing act of generalization. But we have even more than this—a direct contradiction of his own philosophy as it respects the *very important* tests of color, by which our author comes at last at the conclusion that the foregoing lesions are owing in 'the typhoid fever,' at different times, to exactly opposite pathological conditions. Thus: '*Paleness of inflamed structures takes place sooner or later, as is exemplified in the various shades of color of hepaticized lung.*' 'It ought to be noticed, that continuous with a red and softened portion of mucous membrane, we often find another equally softened, but *without redness*. *If the first, therefore, is inflammatory, it is probable that the other is also.*' Here, too, he allows that 'thickening of the sub-mucous cellular tissue' was 'an evident result of inflammation,' although 'recent' and 'retaining its natural paleness.' Whereupon, our author lays down a rule which it was convenient to abandon in expounding the lesions of 'the typhoid affection.' Thus: 'This fact [the foregoing], with *many others*, shows that the thickness of our tissues is one of the most important circumstances to be noticed, and that to *confine ourselves* to the *description of the color* of membranes is often *useless* and even *a cause of error to those who might draw conclusions from imperfectly described facts.*'\* And again, in his *Preface*, '*redness*, considered by itself, offers much less interest,' than '*thickening, softening,*' &c.

"Such was the opinion of our author when reasoning abstractedly

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\* On Phthisis, sec. 135, 136.



upon the results of inflammation, in his work on Phthisis. But, he was also simultaneously engaged about 'the typhoid affection;' and hence we have in the work on Phthisis some ambiguous conclusions as to the dependence of the foregoing lesions of structure upon 'exactly opposite conditions of disease,' as their color might happen to be *red* or *white*. When, however, we come to the work on Typhus, the obscurity is cleared up; and, in a general sense we are told that *red* and *white* must be taken as the ground of an absolute distinction between the pathological causes of such lesions of structure as may be otherwise in all respects alike, and characterized by the same vital phenomena. Nevertheless, it was important to attempt a consistency of doctrine with what had been laid down in the work on Phthisis; and this could readily be done by those occasional guarded contradictions which give to an author a reputation for candor that more effectually secures to him a successful propagation of his favorite, though conflicting hypothesis. Having said this, we are now bound to cite an instance in illustration; and this we do the more readily, as it exhibits, in connection with what we have hitherto quoted from our author, the true foundation of the new philosophy in respect to inflammation and its products, and explains how far morbid anatomy, and specific objects, have been the source of certain existing collisions with the fundamental laws of nature. Thus, then, our author: 'I refer to the sub-mucous membrane of the large intestine, which was very firm, and at least *six times thicker* than natural, and of a *whitish* color. This *thickening*, we cannot doubt, was consequent upon an inflammation of the mucous membrane, but not recent, for the *white* color is inconsistent with the idea of *acute* inflammation.\*' The reader should here regard in their proper connection and involution, the expressions, 'but not recent,' and 'acute inflammation.'"

Let us now see the reverse of Dr. Paine's picture. Under the title of "Consistence of the mucous membrane of small intestine," Louis says:†

"But what was the nature of this softening? We can solve this question only by comparing together the thickness, the consistence, and the color of the mucous membrane, about which we are now treating. Let us now examine the elements of this question thus brought into comparison with each other.

"In 8 of the 12 cases in which the softening existed to a greater or less degree through the whole track of the intestine, the mucous membrane was pale or greyish; it was more or less red in the others, at the end or in the latter half, or through the whole extent of the ileum, and

\* On Typhoid Fever, Vol. II., page 280.

† Ibid., Vol. I., p. 170.

the softening was not greater in the latter cases than in the former. There was manifest thickening in two cases only, in which the mucous membrane was white, or had merely some pale red spots in some points. What deduction shall we make from these facts? Must we admit that the white and red softening have each their own causes, the one wholly different from the other? that one is of an inflammatory and the other of a different nature? This question, which I stated in another work (*Phthisis*), without being able to decide it, seems to me may now be decided affirmatively, at least, in certain cases. For if it is true that softening is the ordinary effect of acute inflammation, and that, when we find redness, thickening and softening combined, inflammation is certain to have existed, and that when the softening and redness exist without thickening, this is still probable; it is not, by any means, probable when the softening is found under different circumstances, that is to say, without redness and without thickening. Any other view of the subject appears to me incorrect, until it be proved that nature has only one mode of producing the softening of which we are speaking, and the contrary seems to me positively established with regard to the softening with diminished thickness of the mucous membrane of the stomach, and of the corresponding cellular tissue, as we have stated previously, and, as we shall see shortly, this is the case in other organs, in many cases. I am far from admitting, therefore, that the softening of the mucous membrane of the small intestine is always inflammatory; on the contrary, it seems to me necessary to admit that it is of an entirely different character in certain persons."

Again, in connection with the above, he says (Vol. 1, p. 170), "One of these, redness, thickening, or softening, is not, when alone, sufficient to prove that inflammation existed."

Compare these quotations from Louis's works with what Dr. Paine says of Louis in the quotation above, viz., that "the inductions rest *chiefly* upon the fact that the foregoing alterations of structure are white in one case and red in the other."

Thus we believe that we have refuted the second of our commentator's statements. It is very singular, but almost constantly the grossest misconceptions and misstatements made by our commentator in regard to M. Louis's views, we are able to contradict from some personal conversation we have had with the French physician. Usually it would be scarcely worth while to mention these incidents; but we cannot forbear stating the following, as it is so directly connected with our subject. We had read a paper in his presence, and in describing the state of the

mucous membrane, we stated merely the *color*. Louis, with his characteristic frankness, declared “that *color alone indicated nothing*—we ought to have described the thickness, consistence, &c., of the part, and therefore our description was really of no kind of value.”

But we would ask whether Dr. Paine, learned as he is, is really willing to say that softening must always be the effect of inflammation. Let him read John Hunter and Dr. Carswell, and he will have proof enough to the contrary; and he will find that softening of mucous membranes is not an uncommon result of causes wholly the reverse of inflammation.

We pass over many pages, and come to the following passage. Dr. Paine says (p. 728), “But, says our author, ‘the tongue was almost always natural.’ Now this, and all that follows in immediate connection with it, is contradicted by every one of his exemplifying cases.” The last part of Dr. Paine’s remark is very true, because, forsooth, he has misquoted Louis, and the facts in Louis’s book, of course, do not agree with his commentator’s false statement. But let us see the facts. Louis states the results of his examination of the tongue to have been as follows: “It was almost always natural,” in 19 cases; almost constantly of a more or less vivid red at its edge, rather frequently dry at apex and centre, in 9 cases; almost constantly dry and coated brownish, but rarely of a bright red, in 8 cases; more or less thickened, cracked or furrowed deeply, in 3 cases; and was covered with a pultaceous whitish coat, in 2 cases.” In other words, in more than half the cases the tongue was *unhealthy*. Dr. Paine’s own carelessness or want of candor has led him into an important error. At the commencement of the chapter, Louis says—“Elle fut presque constamment dans l’état naturel; c’est à dire, sang rougeur, humide, et quelquefois seulement jaunatre et blanchatre chez dix neuf sujets.” In Bowditch’s translation we find it thus—“*It was almost always natural*; that is, it had not any unnatural redness, it was moist, and was at times only a little yellowish and whitish in nineteen patients.”

What can we think of the candor of such a writer, when he takes the first line of a sentence and makes pages of commentaries upon an isolated assertion, without ever deigning to look at the context to learn the real meaning of the author? Our commentator may complain that the sentence was badly constructed. We affirm that this is no excuse; for, in the original, the meaning is perfectly plain, if the punctuation in the translation is faulty. Moreover, if Dr. P. had taken the trouble to read through the chapter, which evidently he never has done,



he would have found that Louis tells him, "the tongue was natural or nearly so in a little less than half the subjects." (Vol. 2, p. 73.)

When a man proves false in one position, we suspect him in regard to others; but we have already quoted enough to prove that Dr. Paine was utterly unfit for the task he has undertaken—that nothing but a great want of self-knowledge, to use the mildest term, could ever have induced him to throw out before the world such criticisms as now lie before us. We may seem to be unduly severe, especially as our commentator tries to be very good humored, nay, almost witty, and says he has "laughed at" the various errors that are prevailing, and yet he "has no cold-blooded envy of their champions." Yet we think there is something more than raillery in such expressions as the following, which Dr. Paine uses when speaking of Louis, viz., "he is warped by an ambition which knows no road to fame but over the ruins of others," &c. The duplicity of "the Frenchman" is likewise perpetually rung in our ears. How very probable it is that a man would throw aside his practice and devote himself for seven years to a task for which ridicule was for a time his sole reward—how very probable, truly, that such a man wished to play a double part; and "that nothing fats him but other men's ruin"! Yet such are the epithets bestowed, with no sparing hand, upon Louis by Dr. Paine.

Having thus far acted on the defensive, we feel disposed to try the opposite course, and shall quote some specimens of the results of our author's mind as applied to medical researches. He lays down his propositions, however absurd they may be, as if he meant, without further ado, to have all men reverence them as the *ipse dixit* of a great father in medicine. Observe, for instance, the following: "Place side by side, the victims of enteritis and typhus fever, and, forgetting the tokens by which they were once distinguished, our scalpel may reveal nothing but one perfect coincidence in morbid lesions. Each may have his 'rose-colored, lenticular spots,' his 'sudamina,' his 'ulcerated epiglottis,' his '*specific* alteration of the glands of Peyer,' his 'intestinal perforations,' and even his 'meteorism;' all, and each one of which, being assumed as pathognomonic of typhoid fever, places our science, according to our author, in a state of 'infancy.' *And so our author farther on.*" (P. 710.)

What consummate folly! What insufferable dogmatism! We want nothing more than this to prove that whatever our author may be as a reader, he has had little practice in medical investigations, and that his learning is of the closet. The passage, however, needs no commentary.

Dr. Paine has completely answered himself—and perhaps the citation of this passage would have been a sufficient *review* of the whole book. To our mind it shows most conclusively the exact character of Dr. Paine's intellect.

We cannot forbear, however, quoting one or two passages more, in order to expose still farther the peculiar character of Dr. P.'s commentaries. So desirous is he, and so confident is he likewise, of "sapping the foundation of the numerical system," that he declares that not one of Louis's cases are entitled to the least credit—because, forsooth, Louis had to trust to the memory of his patients in regard to the early history of their cases. Now really this is almost silly, and if our commentator were not so *very learned*, we should apply the epithet to him. Pray did not Louis see and examine the patients daily after entrance? Moreover, suppose Dr. Paine had been the observer—how would he have learned about the previous history, except from the patient and his friends? The argument, as is unluckily the case with others used by the Dr., goes too far; it is a two-edged sword, and applies as well to his own methods as to Louis's. Perhaps Dr. Paine, with his favorite rule of analogy, would determine from the latter symptoms of disease, what the earlier ones were! We have said the argument is a two-edged sword—but to carry out the metaphor, we must add that it is very dull at both edges. Really we wonder at the folly which led him to use it. But supposing every one of Louis's cases were inaccurate, we cannot see how the "foundations of the numerical system" are "sapped" thereby. The system does not rest upon Louis or any other man. Thank heaven, no truth rests upon one man alone!

But again—and to this extravagance of our commentator we beg special attention. Will it be believed that any man at all acquainted with the history of fever could be so strangely deluded as to say that those phenomena which have been presented to the world by Louis as the anatomical characteristics of fever, and confirmed as such by men like Chomel and Jackson—can it be believed, we repeat, that any intelligent American would state that these lesions cannot be known, because all the organs in Louis's cases were in a state of putrefaction when the examination was made? Had any one asserted this in our presence, we should have considered him either as a madman or a jester. Yet Dr. Paine has actually printed it—as the following quotation proves. "And we purpose showing, by his statement in this particular, that his cases cannot be allowed to form the basis of any of the pathological conclusions at which he arrives. The objection consists in the lateness



of the period after death at which cadaverous examinations were made ; since absolute putrefaction must have advanced considerably in most of the subjects, and ' meteorism ' must indeed have become formidable." (P. 798). After this remark, follows a table, whereby it appears that Louis allowed (average time) 29 hours and a little over to elapse before making his autopsies. It is true that the commentator applies this reasoning to certain views which *he says* our author has about the non-inflammatory character of the lesions—but the argument proves too much, viz., that the observations themselves were wholly worthless. If, as the Dr. says, he has before proved that Louis " had no just knowledge of the symptoms " (p. 798), and now is satisfied that the post-mortem appearances are nothing but " such a light as putrefaction breeds " (p. 799), why then we must yield the point, and confess ourselves humbled and covered with shame—for we have really believed that though Louis has some errors like other men, still he is not a complete dunce. Moreover, he has noble companions—Chomel, Jackson, Hale, &c., for they have arrived at the same conclusions that Louis has in regard to the pathological appearances. How utterly absurd for Dr. Paine to write thus ! Louis knows nothing of the symptoms of fever, and as for his pathology, it is all dependent upon putridity ! Most men would have been afraid to make such bold assertions. Old Æsop comes up before us, and suggests as an illustration of the relative position of our commentator and the object of his criticism, the poor conceited frog and the ox. The former, foolish thing, burst himself when at the height of presumption. So it seems to us that our commentator has done by this last specimen, this climax of his folly.

Finally, we must thank Dr. Paine for one thing, viz., his long-continued protest against an error which we allow is gaining ground, the neglect of the rational signs, while very great attention is paid to the physical ones. This is very pernicious, and needs a protest from every lover of truth. But we do not accuse Louis of being the author of this error. It is the fault of the age. Louis examines with minute care every symptom he can think of, and no one who was acquainted with him would ever accuse him of doing otherwise.

We have now finished a catalogue of a few of the unfair statements and strange dogmatisms of Dr. Paine. We have sometimes regretted that we had undertaken the task, for we have felt that by his extravagances and his Dr. Pangloss method of quoting, he would bring " the bane and antidote " in his own pages. The only circumstances that induced us to do thus much, were those misstatements of Louis's views



which exist throughout the Commentaries, and of which we have given to the reader merely a portion. We might make more numerous quotations, and likewise might enter into a labored defence of the numerical method ; but inasmuch as that method would not be materially interfered with, even if all that Dr. Paine says of Louis's works were true ; inasmuch, moreover, as Dr. Paine does not in fact bring a single argument against it, except what he calls the hasty generalizations of one man—we shall say nothing upon the subject. We wish, however, distinctly to state that we coincide entirely with these remarks by Dr. Jackson, in his Appendix to Louis's pamphlet on Bloodletting, page 170—and we extend them to all of Louis's works ; least of all, however, to his last, viz., on Typhoid Fever.

“In conclusion, many readers may ask if it is thought that the researches, of which this volume contains the results, are to be considered as leading to any positive conclusions. Certainly not. M. Louis has done us great service in stating his own accurate observations. They must have great weight in the minds of reflecting men. We have added all the observations that we have of sufficient accuracy to be compared with his, which will be received for what they are worth. The whole are to be regarded as materials, to which others are solicited to make additions from time to time ; that, at length, so many cases, impartially collected, may be brought together, as shall justify entire confidence in the inferences to be made from them. Ten hospitals, under the care of honest physicians, may settle the questions discussed in this work within five years, so that our posterity will not for ages be able to make any material correction in the answers. Seasons and epidemics will vary, no doubt ; but the general laws will be found the same, and little else would remain for future ages than to settle the allowance to be made for disturbing forces.”

